

the known specimens have been met with in the Upper Shiré district, except those procured by Capt. Alexander on the Zambesi (see *Ibis*, 1900, p. 431).

14. *GLAUCIDIUM PERLATUM* (Shelley, Cat. p. 142).

Shiré R.; ♂ ♀, Banda. Native names: "*Matawese*" and "*Kaungululu*."

15. *BUBO LACTEUS* (Shelley, Cat. p. 144).

Banda. Native name: "*Linjichi*."

16. *ASTURINULA MONOGRAMMICA* (Shelley, Cat. p. 151).

Shiré R.

#### XIV.—On Moults and Alleged Colour-change in Birds.

By WITMER STONE.

THE article by Mr. Bonhote, which appeared in last year's volume of this Journal\*, leads me to make some reply to his criticisms of my paper on "Moults" (published in the 'Proceedings of the Philadelphia Academy,' 1896), though it seems questionable whether any further discussion will result in a better understanding of the phenomena of plumage-change, since the advocates of both sides hold so tenaciously to their own views.

Rather than repeat in detail arguments that have already been fully expanded, I desire to point out some facts in connexion with the study, and to endeavour to show what has been *proven* by recent investigations.

It will be understood at the outset that, with Dr. J. A. Allen, Dr. J. Dwight, Jr., Mr. F. M. Chapman, and most other American ornithologists, I maintain that all colour-changes in bird-plumage are produced either by actual moult or by abrasion of the tips, and that there is no change of pigment in the feathers themselves.

To consider in the first place the attitude of those who differ from us, we note that the advocates of direct change of pigment have been forced to abandon their earlier standpoint—that most changes of plumage in spring-time were

\* See '*Ibis*,' 1900, p. 464.

effected in this way—for the much narrower claim that the change occurs in some feathers of some individuals of certain species.

On the other hand it has now been *demonstrated* that at least many (and apparently all) individuals of every species of bird in Eastern North America which undergoes a spring change of plumage accomplish that change by a moult. If the same thing is not true of European birds, we have certainly a strange state of affairs!

Now let us consider the manner in which investigations have been carried out. It should be thoroughly understood, in the first place, that the study of moult or plumage-change is one of the most difficult branches of ornithology, and requires much pains and constant care to prevent our jumping to conclusions not warranted by the evidence.

The papers so far published against the colour-change theory by Mr. Chapman, Dr. Dwight, and myself are based upon a careful examination of thousands of specimens, many of them in various stages of moult; while the numbers and data of the individual skins upon which our conclusions are based are recorded, together with the actual condition of the feathers.

Mr. Bonhote's paper is notably lacking in these respects, and he gives us his conclusions without placing the evidence before us. For example he states "From the head of *Larus ridibundus* I have taken at the same time new brown feathers and old feathers *in the process of change*" [italics mine]. What he really took were no doubt particoloured feathers *which he thought were changing*, but which we on the other hand think *were always particoloured* from the time they broke from the sheath, and would remain so until they were shed.

From this example it will be seen that the main difference between us is a *difference of interpretation* of what we see in the specimens examined.

Now as to what has been proven. I claim that any one who carefully studies the articles by Messrs. Chapman, Allen, Dwight, and myself must admit that we have proved

that certain individuals (we claim by inference *all* individuals) of the many species that we have examined accomplish the spring change of plumage by a moult, and that the feathers of the nuptial dress which are alleged to have undergone a change of pigment burst from the pin-feather sheaths exactly the same, so far as colour is concerned, as they are in the breeding-bird. Furthermore, did opportunity offer to demonstrate our views personally to our critics with our series of specimens, I am sanguine enough to think that they would agree with us. One of the principal difficulties attaching to the study of moult is the lack of satisfactory material, and this I think has in many cases led Mr. Bonhote and others astray. Few collectors have preserved moulting birds, because they make such ragged specimens; and in my own experience it has often happened that while I have had scores of examples at hand, not one of them showed traces of the moult that I suspected must take place. Nevertheless, the existence of the moult was always demonstrated when an effort was made to secure specimens at the proper time of year. Have such efforts been made to secure spring-moulting examples of European birds in cases where it is contended that no moult occurs, and, if so, has not the investigator been forced to admit that *part of the plumage* at least was moulted?

An examination of our papers mentioned above will, I consider, also force the admission that every fact so far recorded as observed in a prepared specimen or dead bird is entirely in accordance with the theory of a spring moult, and can be quoted more logically as an argument for moult than for the theory of direct change of pigment.

Mr. Bonhote, while admitting a spring moult in many birds, says: "It does not follow that, because a bird is moulting, a colour-change in individual feathers, be they old or new, is thereby excluded." Very true; if we prove that ninety-nine feathers break from the sheath just as they are in the nuptial dress, we may not be able to prove that the hundredth does not undergo a change, and it is manifestly out of the question to demonstrate how every individual

of a species changes its plumage, but having proved a reasonable number of cases, may we not count our inference legitimate? All scientific reasoning is by such methods. Furthermore, with the change by means of moult proven in so many birds, why should we seek to demonstrate that such a wonderful phenomenon as the alleged "change of pigment" should also take place in the same species? especially since many of the details of such a change as set forth by its advocates are at variance with what we know of the histology of the feather.

On Mr. Bonhote's line of argument we might as well claim that although we know that a large number of crabs increase in size only at definite periods when the old shell has been shed, nevertheless this does not *prove* that *some* individual crabs do not go on growing continuously. I fear, however, that carcinologists would regard this as an unnecessary hypothesis and quite unworthy of serious consideration. Furthermore, they would hardly consider the existence of a series of crabs of graded size as *proof* of this method of growth. A series of particoloured feathers, however, is supposed to prove the alleged change of pigment!

Is it not really the reluctance to overthrow a theory which has been held so long that unconsciously prejudices the adherents of the direct colour-change theory?

I therefore once more earnestly refer those who may wish to investigate this subject to our former papers, and for the present merely intend to consider the spring moult of one form. As Mr. Bonhote suggests that I should extend my studies to the *Limicolæ*, we will take as our example the Sanderling (*Calidris arenaria*), a peculiarly appropriate species, since it is common to both sides of the Atlantic, and is known on both by the same technical name!

The arguments of the colour-change advocates, so far as this bird is concerned, are well set forth by Mr. J. G. Millais, in 'The Ibis' for Oct. 1896, p. 451, and on plate x. he illustrates his theory by feathers taken from birds shot in March (a grey feather), April (one with a dusky centre), May (a blackish feather with white tip), and June (a similar

one with fulvous tip). These, he claims, represent the successive stages through which each feather passes as the grey winter plumage changes to the rufous summer dress.

A series of these birds is now before me from various parts of the Atlantic coast of America and Greenland. Winter specimens (26171, Cape May, New Jersey, Nov. 28, 1878, for example) have the feathers of the back like Mr. Millais' fig. 1 (March), while some individuals also show feathers like his fig. 2. In my March, April, and May birds, in which the change is taking place, I find feathers like his figs. 3 and 4 occurring in numbers, both styles in the same specimen, and of those which are just breaking from the sheath quite as many have brown tips as white, while *all these partly expanded feathers are black-centred*. This certainly shows that these grey feathers are not the early stages of the black, as Mr. Millais would have us believe. Furthermore, in a breeding-bird from Greenland (No. 30197, June 14, 1892) many of the feathers have white tips, although all are more or less abraded. This is additional proof that the white-tipped feathers are always white-tipped (except where they are worn) and not early stages of the rufous-tipped.

I may further state that every Sanderling examined which had been taken in the changing spring-plumage showed abundant partly-expanded pin-feathers, and yet Mr. Millais states that no moult occurs at this time! Is it possible that he did not take the trouble to raise the plumage to look for these new feathers, or did he write this statement when he had not his material before him?

The above facts set forth by me (and more fully elaborated by Messrs. Chapman and Dwight) show conclusively that the dark nuptial feathers *which positively do come in by moult* in March, April, and May, remain precisely as they are from the time they burst from the sheath until they fall out in the post-nuptial moult.

To argue that the presence of a series of feathers on one bird, or several which show a range of colour-variation, is a proof that each individual feather goes successively through

all those variations, is no more logical than to claim that if the pales at one end of a fence are painted red, and those at the other end blue, with the intervening ones graded in shades of purple, we have proof that each one of the blue pales has passed through all the intermediate shades of colour!

A few words must be said in conclusion on the study of birds in captivity. Mr. Bonhote suggests that I should extend my investigations to this field. This I have done to a certain extent for some years past, and in the case of *Dolichonyx oryzivorus*, *Zamelodia ludoviciana*, and other species in which the change is less striking, I find in every individual examined that a spring moult takes place, while there is no indication of a colour-change in the individual feathers.

The difficulties in the way of this method of study are very great, and it is extremely hard, as Mr. Bonhote admits, to study any individual feather. To my mind none of the rare instances yet quoted of change of pigment in the feathers of live birds are at all conclusive, since so many ways suggest themselves in which observers may unintentionally misinterpret what they see, and so many details are lacking in their accounts. One argument which has been quoted in support of the colour-change theory is the effect of a diet of cayenne-pepper in brightening the plumage of Canaries; but the breeders with whom I have talked state that no change is noticed until after the moult, and that it is the new feathers which are affected. Are not the alleged changed feathers in living birds merely new feathers suddenly expanded from pin-feathers which had before escaped notice? The presence of a few of these new feathers and a number of permanently particoloured feathers would apparently furnish all the proof required. Moreover, the fact which I have stated elsewhere that cage-birds moult very irregularly and imperfectly, adds to the difficulty of drawing accurate deductions from their study.

It is my earnest desire that many investigators may be led to pursue this branch of ornithological research, and I cannot but feel that if due weight be given to the points brought



